

Comment on the proposed establishment of availability of *Balintus* d'Abrera, 2001, *Gulliveria* d'Abrera & Bálint, 2001, *Salazaria* d'Abrera & Bálint, 2001, *Megathecla* Robbins, 2002 and *Gullicaena* Bálint, 2002 (Insecta, Lepidoptera, LYCAENIDAE)
(Case 3458; see BZN 65: 188–193; 66: 271–272, 349–351; 68: 206–211; 69: 60–61)

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1. We respond to the recent comments of Robbins & Lamas (BZN 69: 60–61), in which they confuse nomenclature and taxonomy and cling to their erroneous concept that the diagnoses of eight generic names proposed by Bálint & d'Abrera (or d'Abrera solely) in d'Abrera (2001) somehow 'differentiate the type species, not the genus'. Therefore it is not surprising that most of the justified responses of Bálint & d'Abrera on the case (BZN 68: 206–210) were considered by the applicants to be 'not relevant'. In their latest comment they attempt yet again to convince readers that they had 'proposed a solution in which all names that were in use were to be considered to be available and those that were not in use were to be considered to be unavailable.' Evidence as to which names were in use had been presented in their original application.

2. The applicants are of the opinion that the names of Eliot proposed in 1973 in his 'tentative arrangement' shown by us as examples for illuminating this case, are irrelevant because they claim they are governed by the now defunct Second Edition of the Code (1964). Although we brought these examples only to demonstrate how easy it is to question well established names using the methods invented by the applicants, therefore it becomes irrelevant to consider whether they are governed by another, already defunct version of the Code. Nevertheless the relevant article 13(i) of the defunct code regulates that any names published after 1930 must be 'accompanied by a statement that purports to give characters differentiating the taxon'. Consequently the Eliotian names may easily be questioned in a similar way: not the new genera, but their type species, which were accompanied by a statement that purports to give characters differentiating the taxon. Moreover, the argument of the applicants is incorrect, because in any case the most recent Code does regulate all the names, also including those proposed by Eliot in 1973, and any nomenclatural question regarding them. Thus clearly the relevant articles must be cited and referred to from the governing version of the Code, and not from the defunct one (Article 86.3).

3. The applicants wrote that Eliot's wording and our wording was not identical. This argument is a semantic nonsense, as the descriptions of different taxa must be shown to be dissimilarly worded. In our previous response we wrote that Eliot's diagnoses were worded in an almost identical manner to ours therefore those names become easy victims when using the methods of the applicants for rejection, especially when someone wants other names replacing the ones proposed by Eliot.

Interestingly none of these names were questioned by one of the applicants, who authored a list of generic names of the butterflies of the world (Lamas, 2008).

4. The applicants have expressed their view that 'in evident contrast' to the names Bálint & d'Abrera proposed, Eliot's names were 'differentiated' by 'generic characters'. But significantly, none of the editions of the Code defines what a generic character is, and what may or may not be 'in evident contrast', and therefore this is not regulated by the Code. What the Code does regulate are the various technical circumstances for making any newly proposed name available. And (remaining strictly in the present case) it must to that end be 'accompanied by a description or definition' of the taxon proposed. Clearly therefore, it is not the function of any nomenclature per se, to provide judgement as to whether the characters described actually distinguish the taxon proposed, or any other. How any nomenclaturist can then claim to be applying objectively the articles of the Code in distinguishing the diagnoses of Eliot as being generic, whilst those of Bálint & d'Abrera 'in evident contrast' are not, is a great mystery!

5. What is evident: as Eliot did, d'Abrera and Bálint used characters they found on the type species to diagnose the genus and on the basis of this they placed further species in the newly proposed genera when these turned out to be polytypic. The genus-group names proposed in d'Abrera (2001) are individually marked by the required suffix 'gen. nov.' expressing the intention of the author(s) to establish new nominal taxa following the Article 16.1. If the applicants have not noticed the suffixes and because of that, have doubts about the availability of the names, then Article 13.3 of the Code rules more strictly the availability of the genus-group names and concentrates on the fixation of the type species. Moreover because in each and every case there is the clearly indicative suffix 'gen. nov.' it does not make sense to claim, citing the Code, that the description that states in words, characters purported to differentiate the taxon proposed, are describing only the type species, and by extrapolation only the specimen used for the diagnosis. Further, the entry 'congeneric species' in the case of polytypic genera underlines that the description is purported to distinguish all the taxa placed in new combinations. Moreover the reasoning of the applicants is illogical as they claim that 'only' the type species is being described, but not the genus. This is because any generic diagnosis must also fit the type species selected; otherwise the designation of a type species becomes pointless if it cannot serve as an objective reference for nomenclatural intention of universality and stability. Therefore Bálint & d'Abrera (2011) did not reinterpret their original 'verbal description' but have shown by applying the 'implied grammatical subject' method contrived by the applicants to this case, that the applicants were mistaken. We repeat, (a) there is a diagnosis and (b) there is a type species clearly and unambiguously designated for each of the generic names proposed in d'Abrera (2001).

6. The paragraphs we have provided concerning ethical issues were necessarily there to demonstrate that the present case has a subjective background on the part of the applicants. Indeed while the Code of Ethics has no power to rule, the Code does concern itself with ethical matters by making recommendations from time to time, thus tacitly admitting their potential for influence. This is because, being entirely realistic, the Commission has always recognised that ethical issues do and must help to regulate the intentions of persons proposing names for living or extinct organisms.

The website the applicants cited as reference is the intellectual work of one of the applicants and is a checklist of names of Neotropical lycaenid butterfly genera (Robbins, 2004). In that checklist Dr. Robbins expressed wholly or partly different taxonomic concepts for many of the genus-group names newly proposed for Neotropical eumaeine lycaenids an entire three years earlier in d'Abreu (2001). Those novel taxonomic concepts were never published by the applicants in any taxonomic journal, but the matter was discussed in length in their application in spite of the fact that according to the Articles 17 and 18 of the Code this dimension is absolutely irrelevant for nomenclature. The only exception was their concept for the generic name *Annamaria* following the revision of Bálint (2005).

7. The hitherto published use of the generic names *Annamaria* (see Bálint, 2005; Robbins & Duarte, 2008; Bálint, 2010), *Megathecla* (see Faynel, 2009) and *Riojana* (see Bálint, Kertész & Wojtusiak, 2006) show that the original definitions of these names are sound and applicable from a nomenclatural point of view. Moreover there are many cases where the species diversity (lists of individual species names) and generic concepts (contents of names) expressed in the checklist compiled by Robbins (2004) and the website mentioned above, and the most recent results diverge, e.g. Bálint, Kertész & Wojtusiak (2006); Duarte & Robbins (2010); Prieto (2008); Robbins, Busby & Duarte (2010); Robbins et al. (2012), etc. Therefore, we ask how do universality and stability become threatened if the names proposed by the applicants for rejection continue to appear unambiguously as given in published books, checklists and papers dealing with Neotropical eumaeine lycaenid classification, faunistics, or taxonomy? On the contrary, those 'generic concepts' of Robbins (2004) have nothing to do with nomenclature sensu stricto, nor with stability. In sum: the arguments of the applicants cannot serve the stability of nomenclature. The names they would like to suppress are well established and have been in universal usage since 2001, so their application is incorrect and redundant.

Additional references

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Comments on the proposed conservation of usage of the specific name of *Scarabaeus fimetarius* Linnaeus, 1758 (currently *Aphodius fimetarius*; Insecta, Coleoptera, SCARABAEIDAE) by designation of a neotype

(Case 3579; see BZN 69: 29–36, 128–140, 221–229)

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Wilson (2001) published her results on karyology of a widely-distributed taxon which so far had been treated as *Aphodius fimetarius* (Linnaeus, 1758). She concluded that this taxon included in fact two different species and designated a lectotype for *Scarabaeus fimetarius* from the original Linnean type series. Unfortunately, Linnean type series was a mixture of species and she has chosen as a lectotype of *S. fimetarius* a specimen belonging to *Aphodius foetens* (Fabricius, 1787), a quite different species easily recognizable by its red abdomen.

Angus et al. (BZN 69: 29–36) and Fery (BZN 69: 128–136) presented two possible solutions of a nomenclatural problem that had arisen from splitting of *A. fimetarius* and the inappropriate designation of its lectotype.

Both of them asked the International Commission of Zoological Nomenclature to use its plenary power to set aside all previous type fixations for the nominal species *fimetarius* Linnaeus, 1758 as published in the binomen *Scarabaeus fimetarius*. If the lectotype is set aside, a neotype designation will be required. And there is the main difference between Angus et al.'s (BZN 69: 29–36) and Fery's (BZN 69: 128–136) concepts. Angus et al. nominates a specimen from 'England, E. Kent' of which Wilson (2001) studied the karyotype, while Fery suggests one from the original Linnean type series – that with number LIN 3386. Because these proposed specimens belong to different species, designation of either of them will differently affect usage of the name *Aphodius fimetarius*. It should be noted, that both concepts are formally correct. Nevertheless, we would like to support the solution proposed by Fery in the interest of nomenclatural stability, as:

– the name *A. fimetarius* will be used for a species which Fery called 'with red elytra' (and not 'with yellowish-red elytra'), which has, according to the current knowledge, a more northern distribution and is the only species occurring in Sweden, the generally assumed locus typicus of *Scarabaeus fimetarius* (Bellmann et al., BZN 69: 136–138; Rößner, 2012).

– contrary to Angus et al. (BZN 69: 29–36) and Barclay (BZN 69: 139–140) we are not convinced that the usage of the name *Aphodius pedellus* (De Geer, 1774) in the sense of Wilson (2001) and Whitehead (2006) is widely accepted by the 'community' since 2001. According to Fery's concept, however, *A. pedellus* would remain a subjective junior synonym of *A. fimetarius*, as it was accepted for more than 250 years.

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We support the Comments on Case 3579 given by Bellmann, Hillert & Rössner and by Fery (BZN 69: 128–138) and ask the Commission to set aside the lectotype of *Scarabaeus fimetarius* Linnaeus, 1758 designated by Wilson (2001) and designate instead a neotype from the remaining paralectotypes in the Linnaeus collection number LIN 3386 which bears a label ‘*Aphodius pedellus* (DeGeer), C.J. Wilson det. 2001’.

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To date eleven comments to our Case 3579 have been published. Of these 11 comments, seven support the proposal and four oppose it. It is these opposing comments that are addressed here.

1. Fery (BZN 69: 128–136) begins by stating that the name *fimetarius* refers to a ‘species with red elytra; more northern distribution in Europe’ and has been used ‘for more than 250 years by a large majority of authors’ while the ‘species with yellowish-red elytra; more southern distribution in Europe’ has been treated by most authors as a colour variant of *fimetarius*, but as a separate species, *A. cardinalis* by Reitter, 1892. Maté (BZN 69: 225–227) shows that the general treatment of *Aphodius fimetarius* in the literature has been as a species with red elytra but with some colour variation. There is no suggestion of a red form as against a yellowish red form.

Likewise Bellmann et al. (BZN 69: 139–138) and later Rößner (2012) misinterpret and misstate the historical use of the name *Aphodius fimetarius* by assigning all historical literature records not explicitly referring to the variatio or aberratio *cardinalis* to this one species that he calls *A. fimetarius* (= *A. pedellus* sensu Case 3579). Rößner (2012) did not list any literature references for Eastern Germany for *A. cardinalis*. It is highly unlikely that such a conspicuous species has never been recorded in a well-collected region. It is rather obvious that both species have been recorded in the literature under the name *A. fimetarius*.

All authors before Wilson (2001) and most authors after Wilson have applied the name *Aphodius fimetarius* to a composite species. This may be amplified by a few relevant examples. Fery himself had labelled his proposed neotype of *Aphodius cardinalis* Reitter originally as ‘*A. fimetarius* L.’ (BZN 69: 133, Fig. 1c), not as anything different. G. Dellacasa & M. Dellacasa (2006) use for their main illustration of *A. fimetarius* (Fig. 1138, p. 404) a specimen from Calabria. Material from so far south is almost certainly *A. fimetarius* as interpreted in Case 3579, and the illustration of the aedeagus (Fig. 1140) supports this view. The Dellacasas’ Figs 1141–1149 illustrate a range of colour variation but with no suggestion of a distinct form with yellowish red elytra. Costessèque (2005, p. 55) also figures the aedeagus of *A. fimetarius* as abruptly downturned, rather typical for the light coloured species, and gives the distribution as everywhere in France, without mentioning a second form or species. According to Charrier (2002) the elytra of *A. fimetarius* are from brownish red to yellowish red (‘Élytres allant du brun rouge au jaune rouge’) indicating that he referred to both *A. fimetarius* and *A. pedellus*. It is worth noting that Balthasar (1964, p. 364), working in Central Europe, where *A. pedellus* is the dominant species, described the elytral colour as yellow-red to pale reddish brown, sometimes with an indistinct darker foggy patch, but very rarely the elytra are black (‘Flügeldecken gelbrot bis hell rotbraun, zuweilen mit undeutlichem dunklerem Nebelfleck, sehr selten sind die Flügeldecken schwarz’). This is very similar to the range of variation illustrated by Dellacasa & Dellacasa. According to Janssens (1960, p. 185f), the elytra of *A. fimetarius* are red, brownish red or reddish yellow (‘élytres rouges, brun-rouge ou jaune rougeâtre’). Panin (1957, p. 173) describes a similar colour range (‘Elitrele [...] roşii sau roşii-gălbui’ – elytra red or yellowish red). Schmidt (1922, p. 272f) considered *Aphodius fimetarius fimetarius* as having ‘Flügeldecken bräunlich- oder gelbrot’ (wing cases brownish or yellowish red), but listed another subspecies, *A. fimetarius subluteus* as having the elytra yellowish red, occurring together with the nominal subspecies. He lists *A. fimetarius cardinalis* from Syria, Algeria, Andalusia, and France with full-length fourth elytral interval and all intervals being keeled at the apex, without referring to colour differences.

Max Barclay (BZN 69: 139–140) notes that although both species are widespread, *A. fimetarius* is more southern and western than *A. pedellus*, and that authors are likely to think of the typical *A. fimetarius* as the species usual in their geographical areas. This is borne out by comparison of the figures of the aedeagi of *A. fimetarius* given by Paulian (1959, Fig. 225), which shows the abruptly downturned parameres shown by Rößner to be typical of *fimetarius* in the sense of Case 3579, and by Bunalski (1999) for the Central European fauna, where his Fig. 78A clearly shows the more gently downturned apices typical of *A. pedellus*.

It is thus clear that published references to *A. fimetarius*, prior to the recognition of two species by Wilson (2001), referred indiscriminately to both the species with no clear tendency towards either of them. In fact, as is indicated in Case 3579, all authors who have followed Wilson (2001) in recognizing that there are two species with red elytra and black abdomens have used the names *A. pedellus* and *A. fimetarius* in the sense proposed by Wilson and maintained by us in Case 3579. This usage has continued since we submitted the Case (Beynon et al., 2012a, 2012b; Mann, 2012; Molander, 2012), with the exception of Rößner (2012) whose detailed faunistic monograph of Eastern German SCARABAEOIDEA is now published. Rößner uses *Aphodius fimetarius* as the valid name for *A. pedellus* and *A. cardinalis* as the valid name for *A. fimetarius*. Rößner (2012, p. 138) used *A. cardinalis* because he could not determine in time for print whether the older name *A. subluteus* Mulsant referred to this species and needed to be used.

Fery's (and Rößner's) proposed nomenclature reverses the usage of those authors who have recognized the existence of two species comprising *A. fimetarius*, for no good reason, being based to a large extent on unproven (and to some extent unprovable) interpretation of earlier published literature.

Fery goes on to suggest *A. cardinalis* Reitter 1892 as the best name for *A. fimetarius* in the sense of Case 3579 because Reitter's description 'is the most precise and the one which fits best the characters of the species with yellowish-red elytra', and proposes a neotype to support this. He dismisses Mulsant's older, available name *A. subluteus* Mulsant, 1842, which he suggests should be set aside. *A. subluteus* was described as a variety with yellowish-red elytra, therefore most likely being identical with our *A. fimetarius*. Apart from the effort required to fix the identity of *A. subluteus* (by designation of a neotype) there seems no good reason to setting it aside. He also suggests setting aside another name older than *A. cardinalis*: *A. nodifrons* Randall 1838 (from Maine, USA), again to avoid identifying it.

We do not consider Reitter's species in any way preferable to possible older synonyms, because Reitter's species concept included only southern populations of the species. Reitter had established *A. cardinalis* for a species from Asia Minor and southern Europe. He had never mentioned its occurrence in Central Europe, where he lived. As an insect merchant, he would have taken the advantage of having for sale in numbers a species that he previously described. Therefore Reitter's concept of *A. cardinalis* was incomplete, and his concept of *A. fimetarius* was the same as any other author's concept before Wilson: a mixture of *A. fimetarius* and *A. pedellus* (sensu Wilson). That he did not recognize *A. cardinalis* in Central European material can certainly be explained by his working procedures: he wrote his keys rapidly and, by using only his 'Handsammlung', a reference collection containing a few specimens of each species (Heikertinger, 1944, p. 93) which didn't allow assessment of intraspecific variability. Since Reitter never published *A. cardinalis* from Central Europe, he almost certainly mixed the two species and identified both *A. fimetarius* and *A. pedellus* from that region as *A. fimetarius*. Reitter's (1892, p. 186) description of protruding genae in *A. cardinalis* ('die Augenlappen treten starker vor'), being a character typical for *A. pedellus*, shows that he had no good understanding of the variation within and between the species in question. Müller (1902), being aware of the variability in the length of the intervals at the tip of the elytra, Reitter's primary diagnostic character for *A. cardinalis*, considered *A. cardinalis* and *A. fimetarius* identical.

Fixing a neotype for Reitter's *Aphodius cardinalis* based on the information given by Fery is problematic. Although Reitter's collection went to the Hungarian National Museum in Budapest, parts of it were sold previously to a wealth of collectors in different countries. Reitter's son took over his father's insect business, including his 'Handsammlung' (Fleischer, 1929), the majority of which ended up in the Sleszke Museum in Opava, and a smaller part in the National Museum in Prague (Horn et al., 1990). Fery gives evidence that type material of *A. cardinalis* is absent from the collections Budapest, Prague, Munich, and Berlin, but he doesn't give evidence whether he has checked with the Opava museum, or with any other large European museum where Reitter's material might have ended up. Reitter's types might well exist. A hasty neotype designation is inappropriate without further research and the documentation of such. However, more research is necessary to determine the species identity of the two older potential synonyms of *A. cardinalis*, *A. nodifrons* Randall, 1838 and *A. subluteus* Mulsant, 1842:

Aphodius nodifrons Randall, 1838, was described from Maine, U.S.A. as having 'bright reddish' elytra. In the Latin description, the phrase 'elytris rufo-sanguineis' was used. Randall's collection is apparently lost, but he gave specimens to Thaddeus William Harris (Sprague, 1875), whose collection went to Boston Society of Natural History, and from there to Boston University (Johnson, 2004), and partly to the Museum of Comparative Zoology in Harvard (J. Traniello, in litt. 2011). FTK is in the process to determine whether type material of *A. nodifrons* still exists.

A. subluteus Mulsant, 1842, was described as a yellowish red variant of *A. fimetarius* from France. Type material of this species has to be considered lost (Paulian, 1944). Mulsant's name has occasionally been used for lighter coloured specimens of the nominal species *Aphodius fimetarius*, at infrasubspecific (e.g. Endrödi, 1956, p. 43; Panin, 1957, p. 174) and subspecific level (Schmidt, 1922) and cannot be considered a nomen oblitum. Because of the colour being described as yellowish red, *A. subluteus* is likely to be a junior synonym of *A. fimetarius* sensu Case 3579 and a senior synonym of *A. cardinalis*, but different from *A. pedellus*.

2. Bellmann, Hillert & Rössner (BZN 69: 136–138) favour designating a Linnaean specimen as neotype for *A. fimetarius* on the ground that material is now identifiable on morphological characters, and this preserves current usage. The ambiguity of current usage has already been discussed. As to identifying material on morphological characters, this is broadly true, though there are difficult specimens. The arrangement proposed in Case 3579 involves designation of a neotype whose chromosomes are known, removing any possible doubt of its identity.

Furthermore Bellmann et al. claim that Gordon & Skelley (2007) hesitated to split *A. fimetarius* into two species in their pivotal monograph of North American APHODIINAE, but they simply state that Wilson's work came too late to be considered: 'By the time Wilson's study was published, all '*A. fimetarius*' specimens used in this North American project had been returned to various lending institutions. Therefore we cannot validate their identity based on their morphology and both karyotypes may occur in North America. For now, we follow the conservative approach and consider them all to be *A. fimetarius*.' (Gordon & Skelley, 2007: 106). Bellmann et al. also claim that 'some authors after 2001 interpreted Wilson's results meaning exactly the opposite' and present Dellacasa & Dellacasa (2006) as an example. This is wrong.

Neither M. Dellacasa & G. Dellacasa (2006), nor G. Dellacasa & M. Dellacasa (2006), nor any other author has interpreted Wilson's results in the opposite way. Many have not accepted the existence of two distinct species, but the only author in the primary literature (outside BZN) recognizing two species within the former *A. fimetarius* and interpreting *A. fimetarius* differently from Case 3579 is Rößner (2012), published half a year after Case 3579. With Rößner's work and Fery's comment (BZN 69: 139–140), we now have the unfortunate situation that the name *A. fimetarius* is applied to two distinct species. Before, the name *A. fimetarius* was either used for a composite species (*A. fimetarius* + *A. pedellus*) or for *A. fimetarius* sensu Case 3579.

3. M. & G. Dellacasa (BZN 69: 221–222) favour the solution proposed by Fery as they consider that it preserves current usage and thus promotes stability. The ambiguity of current usage of *A. fimetarius* has already been discussed at length – this argument fails. The Dellacasas further suggest that the type locality of *A. fimetarius* should be 'conventionally restricted' to Sweden, but the publications they quote in favour of this are prebinominal and thus inadmissible. Moreover, parts of northern Germany where *A. fimetarius* sensu Case 3579 occurs (Rößner, 2012, p. 139) belonged to Sweden in Linnaeus's times (Hacker & Hardenberg, 2003). The type locality and type material of *Scarabaeus fimetarius* L. are discussed in detail in paragraph 6 of Case 3579. The type locality, as stated by Linnaeus, is 'Europe' and Linnaeus lists German material as well as his own. There is thus no reason to reject the concept of *A. fimetarius* outlined in Case 3579.

4. Branco (BZN 69: 228–229) begins by reference to Landin's settling the identity of *A. foetens* (Fabricius), then suggests that Wilson could not recognize *A. foetens* and that the pronotal and head characters she gives for *A. fimetarius* (as against *A. pedellus*) are based on confusion with *A. foetens*. This is not true. The reason Wilson (and Angus) failed to recognize the Linnaean *A. foetens* is that they did not look at its abdomen, as is pointed out in Case 3579 where, incidentally, it is shown that Landin himself also failed to spot the Linnean *A. foetens*. In her 2001 paper Wilson refers to the methods for obtaining chromosome preparations, citing Shaarawi & Angus (1991). The methods described here involve removing the abdomen – not possible without looking at it! Wilson and Angus (2004) describe the karyotype of *A. foetens* as well as those of *A. fimetarius* and *pedellus*, along with *A. foetidus* (Herbst) and *A. conjugatus* (Panzer). The remainder of Branco's comment is based on this complete misinterpretation of Wilson's work.

Conclusions

No concern was shown by any authors when Wilson established her concepts of *A. fimetarius* and *A. pedellus* apart from the fact that some did not accept the splitting of the species *A. fimetarius* by rejecting or disregarding chromosomal evidence. The assignment of the names *A. fimetarius* and *A. pedellus* to the two species in question has not been criticised for a decade. Now that we initiated correction of the type selection for *A. fimetarius* to match these very concepts, suddenly protests emerge.

There is agreement that Wilson's lectotype designation needs to be suppressed and a neotype needs to be designated. There is no agreement, and in our view no sound argument for transferring the name *A. fimetarius* from *A. fimetarius* sensu Case 3579 to *A. pedellus*, and to replace the name *A. fimetarius* (sensu Wilson) by a name that

probably has available senior synonyms. Before 2001, *A. fimetarius* has always been applied to a composite species, then this composite species was split and *A. fimetarius* applied to one of the species (Wilson 2001). All those authors who accepted the split have followed Wilson (2001). Now that more authors have recognized that the former composite species is indeed composed of two distinct species, some want to apply *A. fimetarius* to the other species. This would be a source of nomenclatural instability and major confusion, particularly since we are dealing with two of the most abundant Holarctic beetle species.

Additional references

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This comment is a response to several comments published in BZN 69(3): 221–229. Almost all comments which support the application of Angus et al. refer to my use of the terms ‘species with red elytra’ and ‘species with yellowish-red elytra’ although I proposed using these terms simply to broadly delineate the two species with a full understanding that ‘the morphological characters sometimes show tendencies to overlap, . . .’ I did not state that the species always have elytra with one of the two colours, or can be determined safely based on this character alone. Unfortunately many comments misunderstood my intentions and criticised this terminology, although I never suggested the colour of elytra as a solid basis for identification. The recently published work of Rößner (2012, pp. 138, 140) gives a large number of characters that can safely distinguish both species on the basis of colour and/or morphology.

Another argument against my Comment is that the name *Aphodius fimetarius* had always been used before Wilson (2001) for both species. Indeed the name was used for both species, with the exception of Randall (1838) and Reitter (1892). All the other authors either ignored the differences or did not consider them sufficient for species separation, although some authors acknowledged the existence of different morphs and/or varieties. Although ‘prevailing usage’ is one of the main arguments in the application of Angus et al., that key term is only superficially dealt with in the comments, probably because these authors agree with me that most other authors have used and are still using the name *A. fimetarius* in exactly the same manner as before Wilson (2001) (i.e. for both species).

The identity of the paralectotypes of *Scarabaeus fimetarius* Linnaeus, 1758 and that of the lectotype and paralectotypes of *Scarabaeus pedellus* De Geer, 1774 is clear and undisputed (see also Angus et al. BZN 69(1)). This lectotype and the paralectotypes of both taxa belong to a single species. The identity of *A. cardinalis* Reitter, 1892 is also clear since the designation of its neotype. Wilson identified karyotypes for specimens that she identified as *A. pedellus* from southern England for the ‘species with red elytra’, and karyotypes for specimens ‘with yellowish-red elytra’ (= *A. cardinalis*; named by her *A. fimetarius*) from Cyprus. Thus, there is no need to designate a chromosomally determined specimen as neotype of *S. fimetarius*. It could be suggested, however, that the karyotypes for these species should be re-examined based on specimens from the type regions Sweden (for *pedellus*) and e.g. Spain (Andalusia, for *cardinalis*). Interestingly, neither the applicants nor any of those who supported Angus’s case asked the Commission to set aside the lectotype designation of *S. pedellus* by Wilson (2001) and to designate instead a chromosomally determined specimen as neotype.

The distribution of neither species has been fully clarified. It is certain that *A. cardinalis* (the ‘yellowish-red species’) does not occur in Sweden (the most probable type locality for *S. fimetarius* Linnaeus, 1758 and *S. pedellus* De Geer, 1774) and most probably not in most parts of European and Asian Russia. This species, however, becomes more frequent in southern Europe and seems to be the only species occurring in Northern Africa and the Near East (and also in much of Italy; pers. comm. by G. Dellacasa). The situation on the Iberian Peninsula is as follows: based

on a large amount of material studied by Rößner and myself both species on the face of it seem to be distributed in most parts of the peninsula; however, a closer inspection shows (in accordance with the assumption of ‘ecological differences’ in Maté’s Comment) that the ‘red species’ is distinctly rarer and prefers higher altitudes, while the ‘yellowish-red species’ is more frequent and prefers lower altitudes. Only two localities (in the western Pyrenees) have been found among the material studied where both species occur sympatrically.

The observation that both species occur in a few cases sympatrically by no means provides any argument against my choice of the neotype of *Aphodius cardinalis* Reitter, 1892, because the type locality has an altitude of about 50 m and all other specimens found here doubtlessly belong to that species. Thus, there is no trace of any ambiguity about the identity of this neotype. There is also no ambiguity about the identity of the paralectotypes of *S. fimetarius* in the Linnean collection. These all belong to the species which I call ‘the one with red elytra’. The identity of specimen LIN 3386 (proposed as neotype for *S. fimetarius*) is not based on a photo, but on that specimen itself, stored in the Linnean collection (cf. the Comment of M. Forshage, BZN 69(3), p. 224).

Maté (BZN 69(3), p. 227), wrote: ‘In his [Reitter’s] description the diagnostic character was not the elytral colour but the particular convexity of the interstriae near the apex (from Schmidt, 1922, p. 273)’. Maté, however, did not cite the original description, and, thus, overlooked the fact that Reitter (1892) in his description mentioned more than one character to distinguish the newly described species from *A. fimetarius* (‘red elytra’). In particular, Reitter expressly indicated the differently coloured elytra of both species. On the other hand he attributed pronounced cheeks to his *A. cardinalis*. It is possible that Reitter in his differential description confused the shape of the cheeks of both species (which can easily happen in such descriptions). I can not exclude that Reitter, when describing his new species, had in his hands a specimen with unusually strongly developed cheeks. It is, however, beyond any doubt that Reitter must have studied many other specimens from a very large distribution area (Syria (‘common’), Algeria, Andalusia), and judging from our knowledge of the distribution of both species, this must have been—except for a few among the Andalusian specimens—exclusively the species with the ‘yellowish-red elytra’. This view is supported by two further facts: (1) Reitter in Heyden, Reitter & Weise (1906, p. 718; Reitter prepared the SCARABAEIDAE part) reported this taxon from ‘E. m.’ (= southern Europe; taxa from the Near East and northern Africa are not included in this Catalogue) as an aberration of *A. fimetarius*; (2) Reitter (1909) made no record of that taxon in Central Europe.

I could have selected from my material a neotype for *A. cardinalis* with more pronounced cheeks, but have deliberately not done so in order to conform as much as possible to Wilson’s material and with what we know today about the external morphology of the ‘species with yellowish-red elytra’.

If the Commission accepts the proposals outlined in my first Comment (BZN 69 p.134) and selects a neotype for *S. fimetarius* from the paralectotypes in the Linnean collection (specimen LIN 3386), then the interpretation of Linnaeus’s *S. fimetarius* (‘the species with the red elytra’) would still be the same as before Wilson’s work, except that now we know about the existence of two species instead of one; the continuity of the principal interpretation of Linnaeus’s species would be maintained

and the risk would be avoided that the name *A. pedellus* is not accepted by the community of dung beetle specialists for the ‘species with the red elytra’. Additionally, the Commission would not deviate from Recommendation 75A of the Code. In twenty years’ time, nobody would remember a short period of 12 years during which a few authors (25 of ca. 450 authors who published about one or both of these taxa in the last decade, see BZN 69, p. 131) tried to ‘revolutionize that situation’ (A. Ballerio, BZN 69, p. 228).

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Comment on *Scarabaeus* Linnaeus, 1758, *Dynastes* MacLeay, 1819, SCARABAEINAE Latreille, 1802, and DYNASTINAE MacLeay, 1819 (Insecta, Coleoptera, SCARABAEOIDEA): proposed conservation of usage
(Case 3590; see BZN 69: 182–190)

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I wish to register my strong support for conservation of the current usage of *Scarabaeus* and *Dynastes* and their dependant higher taxa, as proposed in Case 3590. The case has been thoroughly researched and is clearly presented. As a further illustration of the wide use of *Scarabaeus*, the pioneering and inspirational work of Fabre (1919) may be cited.

Additional reference

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(2) Brett C. Ratcliffe

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I am writing to offer a brief comment in support of the petition in Case 3590 to conserve the current usage of the generic names *Scarabaeus* Linnaeus, 1758 and *Dynastes* MacLeay, 1819. Krell, Branco, and Ziani have constructed a detailed history of the names and demonstrated that strictly following the code would create extreme nomenclatural instability at both the generic and subfamily levels for taxa

that have been widely known and understood for nearly two centuries. One of these genera, *Scarabaeus*, would be replaced by the long forgotten name *Actinophorus*. And the subfamily in which I am a specialist, DYNASTINAE (rhinoceros beetles), would now become SCARABAEINAE, which the entire scarab community knows refers to dung beetles. Even as a devotee of the Code, I and many others would be unlikely to follow these changes if they are allowed to stand, and hundreds, if not thousands of publications using these names would suffer an identity crisis because of a fundamental change in concepts.

I urge the Commission to use its plenary power to conserve the current usage of these names as proposed by Krell et. al.

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I support the application by Krell et al. The consequences which the authors demonstrate on p. 186 of their application are very unwarranted and considerably threaten the stability of nomenclature. This can be avoided by setting ‘. . . aside all type species fixations for the nominal genus *Scarabaeus* Linnaeus, 1758 before that of *Scarabaeus sacer* Linnaeus, 1758 by Hope, 1837; . . .’ as proposed by the applicants (BZN 69:186).

For the understanding of the reader I want to add a relatively small detail which, however, does not affect the principal intention of the application. The applicants state that according to their source (Jolyclerc, 1807a, p. iii) Lamarck was responsible for the text cited above. This might be interpreted as ‘Lamarck himself wrote the respective part in Jolyclerc (1807b)’.

In Jolyclerc (1807b, p. 368) can be found the following text: ‘Ce genre, suivant Lamarck, doit être divisé en scarabés cornus ou épineux, soit sur le chaperon, soit sur le corselet; . . .’ [This genus, following Lamarck, must be divided into scarabids with horns or spines, either on the head or on the pronotum; . . .] and some lines further on: ‘L’insecte donné pour type à ce genre est le scarabé hercule, *scarabaeus hercules*, Lin.; . . .’ [The insect given as type of this genus is the scarabé hercule, *Scarabaeus hercules*, Linnaeus; . . .].

The title page of Jolyclerc (1822, the 2nd edition of Jolyclerc, 1807a) states ‘. . . d’après . . . Lamarck et Latreille, pour les Insectes; . . .’ [according . . . to Lamarck and Latreille, for the insects; . . .]; and on p. iii (Préface): ‘. . . il nous suffira de citer les auteurs . . . Pour les insectes, M. Lamarck; et nous avons ajouté les nouvelles espèces décrites par Latreille . . .’ [. . . we consider it to be enough to cite the authors . . . For the insects, M. Lamarck; and we have added the new species described by Latreille . . .].

The words ‘suivant Lamarck’ (repeated at other places in Jolyclerc, 1807b, although no such author is given in similar situations, e.g. under the entry ‘Scaure, Scaurus’) and ‘il nous suffira de citer . . . M. Lamarck’ are in contrast to the assumption that Lamarck himself has written those parts in Jolyclerc (1807b, p. 368).

In Lamarck (1801, p. 206) the ‘*Scarabaeus hercules*. L. Fabr.’ is given as the single member of genus ‘*Scarabaeus*. L.’ It is likely that Jolyclerc (1807b) cited this part of

Lamarck (1801) and treated the taxon as type species because it was the only one given by Lamarck.

If my suspicions are correct, this would not affect the year, however, the authorship of that type designation might then be considered to be Jolyclerc himself.

If, however, the Commission sets aside all type species fixations for the nominal genus *Scarabaeus* Linnaeus, 1758 before that of *Scarabaeus sacer* by Hope (1837), then this slight difficulty is absolutely unimportant.

I thank F.-T. Krell for valuable information and for sending copies of rare papers.

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Comment on *Hemerodromia* Meigen, 1822 and HEMERODROMIINAE Schiner, 1862 (Insecta, Diptera, EMPIDIDAE): proposed conservation of usage of the genus-group and family-group names

(Case 3589; see BZN 69: 191–194)

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I strongly support the conservation of the current usage of the generic name *Hemerodromia* Meigen, 1822 to guarantee stability of nomenclature in the dipteran family EMPIDIDAE.

Since 1973 I have published more than 130 papers on the family EMPIDIDAE including several monographs and a substantial reclassification of this formerly polyphyletic family (Chvála, 1983). I know quite well what would happen within the nomenclature of the family if the genus name *Hemerodromia* Meigen were not conserved.

Additional references

Chvála, M. 1983. The Empidoidea of Fennoscandia and Denmark 2, *Fauna Entomologica Scandinavica*, Vol. 12. 279 pp. Brill, Leiden, The Netherlands.

Comment on the proposed replacement of unidentifiable name-bearing type by a neotype for *Plateosaurus engelhardti* von Meyer, 1837 (Dinosauria, Sauropodomorpha)

(Case 3560; see BZN 69: 203–212)

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In Case 3560, Galton discussed the taxonomic history of the well-known sauropodomorph genus *Plateosaurus*. In this Case, Galton asked the Commission to designate SMNS 13200 as the neotype of *Plateosaurus engelhardti*. I would like to

express support for Case 3560 for the following reasons, both nomenclatural and taxonomic.

The case is complicated by the fact that there has been no consensus on the taxonomic composition of abundant plateosaurid remains from upper Norian deposits in Germany, with some authors (Galton 2000, 2001a,b; Galton & Upchurch, 2004; Galton & Kermack, 2010) treating *P. longiceps* as distinct from *P. engelhardti*, and other authors (Moser, 2003; Yates, 2003) following Galton (1984a, 1985a) in considering *P. longiceps* and *P. trossingensis* as junior synonyms of *P. engelhardti*. The more recent paper by Prieto-Marquez & Norell (2011) used *Plateosaurus erlenbergiensis* Huene, 1908 (the original description is often listed as Huene, 1905, but the 1905 paper by Huene contained no description and thus *P. erlenbergiensis* Huene, 1905 is a nomen nudum) for all diagnostic specimens from Germany traditionally referred to as *P. engelhardti*, but did not comment on the diagnosability of the *P. engelhardti* lectotype (UEM 552) or discuss differences between *Plateosaurus* specimens from Trossingen and Halberstadt.

The name *Plateosaurus* is well entrenched in the literature and SMNS 13200 has been used as the reference specimen when comparing *Plateosaurus* with other basal sauropodomorphs (e.g. *Massospondylus*, *Anchisaurus*). To otherwise relegate *Plateosaurus* to the status of a nomen dubium would upset nomenclatural stability and further exacerbate taxonomic confusion because, as acknowledged by Galton, UEM 552 is distinct from *P.* (= *Sellosaurus*) *gracilis*, *P.* (= *Gresslyosaurus*) *ingens*, *Efraasia minor*, and *Ruehleia bedheimensis*, but not *sacra* referred to *P. longiceps* and *P. trossingensis*. Since the genus *Ruehleia* Galton, 2001 differs from *Plateosaurus*, and the holotype of *Efraasia minor*, in having a dorsal incorporated into the sacral (Galton, 2001a, b), other putative synonyms of *P. engelhardti* (*Dimodosaurus poligniensis*, *Gresslyosaurus plieningeri*, *G. robustus*, *G. torgeri*, *Plateosaurus reiningeri*, *P. quenstedti*, *Pachysauriscus ajax*, *P. magnus*, and *P. wetzelianus*) should be re-assessed to see if they are conspecific with either of the two species of *Plateosaurus* recognized by Galton & Kermack (2010), or *Ruehleia*. On the other hand, the holotype of *Plateosaurus erlenbergiensis* (SMNS 6014) may be conspecific with either SMNS 13200 or MB.R.1937 as it preserves cranial material to be compared with *P. longiceps* and *P. engelhardti* (= *P. trossingensis*), but further study of SMNS 6014 is required to confirm this. Finally, '*Pachysaurus*' *giganteus* Huene, 1932 still stands as a nomen dubium in Sauropodomorpha indet. (as per Galton, 2001b) because the three holotype fibulae (GPIT E, which almost certainly come from two individuals) are not diagnostic to genus or species level (as per Galton, 2001b).

Given the risks of nomenclatural stability resulting from abandoning use of the name *Plateosaurus*, I strongly support the proposals in Case 3560.

OPINION 2309 (Case 3429)**CHARILAIIDAE Dirsh, 1953 (Insecta, Orthoptera): proposed precedence over PAMPHAGODIDAE I. Bolívar, 1916 not granted**

Abstract. The Commission has not supported the request to give the name CHARILAIIDAE Dirsh, 1953, for a group of African grasshoppers, precedence over the senior name PAMPHAGODIDAE I. Bolívar, 1916.

Keywords. Nomenclature; taxonomy; Orthoptera; CHARILAIIDAE; PAMPHAGODIDAE; grasshoppers; Africa.

Ruling

It is hereby ruled that the application to give the name CHARILAIIDAE Dirsh, 1953, for a group of African grasshoppers, precedence over the senior name PAMPHAGODIDAE I. Bolívar, 1916 is not approved. No names are placed on the Official Lists or Indexes in this ruling.

History of Case 3429

An application to conserve the usage of the name CHARILAIIDAE Dirsh, 1953 for a group of African grasshoppers by giving it precedence over the senior name PAMPHAGODIDAE I. Bolívar, 1916, was received from David C. Eades and Lesley S. Deem (*Illinois Natural History Survey, Champaign, IL, U.S.A.*) on 28 June 2007. After correspondence the case was published in BZN 65: 20–23 (2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 21–22. One Commissioner split his vote, so that proposals 4(b) and 4(d) FAILED, while all other proposals were approved by a majority of Commissioners (11 FOR, 10 AGAINST) but failed to meet the two-thirds majority required for approval. In accordance with the bylaws, the proposals in BZN 65: 21–22 were sent for a revote on 1 June 2012. At the close of the voting period on 1 September 2012 the votes were as follows:

Affirmative votes – 2: Kullander and Zhou.

Negative votes – 21: Alonso-Zarazaga, Ballerio, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Harvey, Kojima, Krell, Kottelat, Lim, Lamas, Minelli, Pape, Rosenberg, Štys, van Tol, Yanega and Zhang.

Winston abstained.

Ng, Patterson and Pyle were on leave of absence.

In the first round of voting the Commissioners commented as follows. Voting AGAINST, Alonso-Zarazaga explained that the authors did not achieve, in his opinion, a full demonstration that CHARILAIIDAE was in prevailing usage. Some of the

references they quoted were of exactly the same kind as that mentioned in their para. 7: 'a list contained within a single sentence' which only demonstrated that authors copy each other. Moreover, the authors mentioned the Orthoptera Species File Committee as the main source for recommendations in Orthopteran names. They were a source as any other, not the ultimate reference. Bisby's references were annual issues of the same database. Both names had been described in the 20th century, and Dirsh, a reputed specialist, missed the taxon described by another reputed specialist, I. Bolívar. Alonso-Zarazaga felt that priority should be applied here as the main principle of the Code, since the taxa had no economic, medical or veterinarian interest. He suggested that since no comments had been received, this indicated that orthopterologists were not interested in this question. He also noted that the name should be correctly written Bolívar, not Bolivar, as written in the application. Also voting AGAINST, Bouchet said he was not very impressed by the list of references given as evidence that the name CHARILAIIDAE had become widely accepted: four references were by the author of the name CHARILAIIDAE himself, three were by one of the authors of the application, which left seven more references by seven different authors. Species 2000 (Bisby et al., various editions), ITIS, GenBank, GBIF and Tree of Life were all interconnected, and if PAMPHAGODIDAE were restored as the valid name this usage would cascade from one database to the next. So this left fewer than ten references in favour of CHARILAIIDAE, versus three (Johnston, 1956; Kevan, 1961; Vickery, 1997) in favour of PAMPHAGODIDAE. Bouchet felt that priority should apply. Also voting AGAINST, Kottelat said the priority of PAMPHAGODIDAE had been known since 1961, but for 48 years the Code had not been followed and the Commission is now asked to endorse the resulting situation. The application did not mention the consequences of strictly following the Code besides adjusting to a new name, something which taxonomists were used to doing. The application did not mention why changing the name of this small family of five species would create problems for taxonomists. He said he assumed that only taxonomists were concerned because the application mentioned neither non-taxonomic references nor any of non-taxonomic significance. The only point Kottelat could see would be the near homonymy with PAMPHAGIDAE, which he again felt was something that taxonomists were used to. Voting AGAINST, Ng noted that the family was very small with very few genera and species and had neither commercial nor other significance. His feeling, therefore, was that the change in family name would not have an impact on biologists in general. While he said he respected the views of the specialist group concerned, he also noted that no one had written in to support or endorse the application. He would therefore prefer to be conservative and strictly follow the Principle of Priority here.

In the second round of voting the Commissioners commented as follows. Both Alonso-Zarazaga and Bouchet voted AGAINST and said their comments from the first round of voting still applied. Halliday explained that he voted AGAINST because he did not believe there was sufficient evidence that reversal of precedence was justified. He felt that the authors had not shown that a simple application of the Principle of Priority would be damaging, except to a small handful of specialists. These were exactly the people who should be able to adapt quickly to the use of the correct valid name for this taxon. Also voting AGAINST, Harvey said he saw no exceptional circumstances in this case that would warrant the reversal of

precedence. The family was very small, with only five species in four genera, and apparently little known outside of taxonomic circles. Also voting AGAINST, Kojima said that there was little evidence was weak supporting significant prevailing usage of CHARILAIIDAE Dirsh, 1953 over PAMPHAGODIDAE I. Bolívar, 1916. Consisting of only five species in four genera, use of PAMPHAGODIDAE instead of CHARILAIIDAE for this group might not cause nomenclatural instability that required using the plenary power. Also voting AGAINST, Štys explained that he felt Principle of Priority was preferable. Also voting AGAINST, Lamas explained that in the first vote he had voted for the proposals but now, after having considered the comments made by Alonso-Zarazaga, Bouchet, Kottelat and Ng, he had changed his opinion and voted AGAINST. He believed strict priority should be followed here.

No names are placed on Official Lists and Indexes by the ruling given in the present Opinion. The issue is left open for subsequent workers to follow the precepts of the Code or to make new proposals to the Commission.

OPINION 2310 (Case 3547)***Cryptotermes dudleyi* Banks, 1918 (Insecta, Isoptera): precedence given over *Calotermes havilandi parasita* Wasmann, 1910 (currently *Cryptotermes parasita*)**

Abstract. The Commission has conserved the usage of the specific name *Cryptotermes dudleyi* Banks, 1918 for an important economic termite pest species by giving the specific name *dudleyi* precedence over *parasita* whenever the two are considered to be synonyms.

Keywords. Nomenclature; taxonomy; Isoptera; KALOTERMITIDAE; *Cryptotermes*; *Cryptotermes dudleyi*; *Cryptotermes parasita*; termites; worldwide.

Ruling

- (1) Under the plenary power, the Commission has given the name *dudleyi* Banks, 1918, as published in the binomen *Cryptotermes dudleyi*, precedence over the name *parasita* Wasmann, 1910, as published in the trinomen *Calotermes havilandi parasita*, whenever the two are considered to be synonyms.
- (2) The name *parasita* Wasmann, 1910, as published in the trinomen *Calotermes havilandi parasita*, is hereby placed on the Official List of Specific Names in Zoology with the endorsement that it is not to be given priority over the name *dudleyi* Banks, 1918, as published in the binomen *Cryptotermes dudleyi*, whenever the two are considered to be synonyms.
- (3) The entry on the Official List of Specific Names in Zoology for the name *dudleyi* Banks, 1918, as published in the binomen *Cryptotermes dudleyi*, is hereby amended to record the endorsement that it is to be given precedence over the name *parasita* Wasmann, 1910, as published in the trinomen *Calotermes havilandi parasita*, whenever the two are considered to be synonyms.

History of Case 3547

An application to conserve the usage of the specific name *Cryptotermes dudleyi* Banks, 1918 for an important economic termite pest species, by giving the specific name *dudleyi* precedence over *parasita* whenever the two are considered to be synonyms, was received from Kumar Krishna (*American Museum of Natural History, New York, NY, U.S.A.*) and Michael S. Engel (*Natural History Museum, University of Kansas, Lawrence, KS, U.S.A., and American Museum of Natural History, New York, NY, U.S.A.*), on 11 December 2010. After correspondence the case was published in BZN 68: 109–112 (2011). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 June 2012 the members of the Commission were invited to vote on the proposals published in BZN 68: 110–111. At the close of the voting period on 1 September 2012 the votes were as follows: